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Professor Struve communicated the result of his observations and measurements of the apparent distance between the body and the ring of Saturn; from which it appears that he is decidedly of opinion that the body of Saturn is not in the centre of the ring. From a mean of 15 measurements, he makes the apparent distance on the left side equal to 11″·272, and on the right side equal to 11″·390: the difference is 0″·215. The probable error of these mean measurements he considers equal to 0″·024. M. Struve adds some slight corrections to his former measurements of the diameters, &c. of Jupiter, of Saturn, and of the Ring; which he has deduced from a more accurate determination of the value of his micrometer.

The next communication was from Mr. Prinsep, of Benares, giving an account of *two* eclipses which he had observed at that place in the course of the preceding year. The *first* was a solar eclipse on April 26, 1827. The commencement was not observed; but during the course of the eclipse a number of micrometrical measurements were taken by means of the five horizontal wires of a Troughton's 18-inch circle; and he states the end of the eclipse to have taken place at $20^h\ 3^m\ 7^s,5$ mean time at Benares. Mr. Prinsep then adds: "At the period marked as the end of the eclipse, the sun's disc was clear of the moon; but, for 10 or 15 seconds later, I remarked, as it were, a stretching of the sun's edge toward the point which the moon had just quitted. This was apparently the effect of refraction by the moon's atmosphere." The end of the eclipse was also observed by Mr. Walter Ewer at Cawnpore, at $19^h\ 56^m\ 3^s,5$ mean time at that place. The *second* eclipse, alluded to by Mr. Prinsep, was of the Moon, on November 3, 1827. It was observed in the same manner as the solar eclipse above mentioned. The following is the result of his observations, stated in mean time:

		h	m	s
The edge became dull at		8	42	0
The edge invisible		8	44	18
The edge of the same colour as the sky		8	45	30
Decided shadow		8	46	1
A bright spot becomes invisible		8	52	26
Immersion of a small star		10	42	14,2
Moon's transit	1st limb	11	45	48,2
Do. do.	2d limb	11	47	57,1
End of the eclipse		12	1	6

Mr. Walter Ewer observed the beginning of this eclipse at Cawnpore, $8^h\ 35^m\ 23^s$ mean solar time at that place: the difference of longitude is assumed equal to $-\ 10^m\ 12^s$. Diagrams illustrating the several phases of the eclipse, and apparently drawn up with considerable care, accompanied these communications.

Mr. Rumker, of Paramatta, in New South Wales, communicated the result of several series of observations in which he had been engaged at the observatory there. They contained, 1st, The determination of the solstice in December 1826, June 1827, and December 1827. In deducing the results, he notices the insufficiency of Delambre's formula for the *reduction to the meridian* in cases of great altitude; and suggests an alteration when the sun culminates near the zenith. 2nd, Observations on the inferior conjunction of Venus in December 1826. 3d, Observations of moon-culminating stars during parts of the years 1826 and 1827. 4th, Places of some of the fixed stars in the southern hemisphere. 5th, A catalogue of stars with which the great comet of 1825 was compared. 6th, Corrected observations of the place of the said comet. 7th, A determination of the latitude of the observatory by reflection with the mural circle.

The next communication was from Mr. Curnin, of Bombay, giving a more accurate account of his observations of moon-culminating stars at that observatory, during the year 1825: from a mean of all which, compared with corresponding observations at Bushy Heath and at Greenwich, he deduces the longitude of the observatory equal to $4^h\ 51^m\ 9^s$ east from Greenwich.

Mr. Baily presented a short account of the two invariable pendulums, the one of iron and the other of copper, which he had caused to be made, agreeably to the order of the Council, and which are intrusted to the care of Captain Foster in his present voyage of experiment and observation, for the purpose of investigating the possible effects of the earth's magnetism in various geographical positions. The form and construction of these pendulums are somewhat different from those in general use; consisting merely of a plain, straight, uniform bar of metal; and, as he conceived that there is a considerable advantage in having each pendulum a *convertible* pendulum, he has placed *two* knife-edges on each bar. This property of convertibility, however, instead of being effected by a sliding or movable weight, is produced by filing away one of the ends of the pendulum, until the number of vibrations on the two knife-edges are equal. The mode of making a pendulum of this kind he then describes in the following manner: Take a plain straight bar of metal, two inches wide, half an inch thick (or $\frac{4}{5}$ths of an inch, if thought preferable), and *about* $62\frac{1}{2}$ inches long; at least it should not be shorter than this, prior to the first trial for the adjustment. At five inches from one end of the bar should be placed the apex of one of the knife-edges, which he calls A, and at the distance of 39·3 inches therefrom, should be placed the apex of the other knife-edge, which he calls B; each knife-edge being firmly and properly secured in the usual manner. The distance of 39·3 inches is chosen, because the intervals of the coincidences are in such case about 15 minutes; but if an interval of about 10 minutes should be preferred, the distance should be about 39·4 inches: and so in proportion.

In order to adjust a pendulum of this kind, it must be placed on the agate planes, and the number of vibrations determined in the usual manner, beginning, for the sake of regularity, with the knife-edge A; then inverting the pendulum, and determining the number of vibrations with the knife-edge B. In these preliminary experiments it is not necessary to extend the observations beyond one coincidence; neither is it requisite to apply any other correction than for the arc of vibration, and for the temperature of the room, since all the other sources of error will be common to the two positions of the pendulum, and therefore may be rejected in these first trials. If it should be found (as will in fact be the case) that the knife-edge B makes a *less* number of vibrations in a day than the knife-edge A, we must file away the bar at the end B, until the two knife-edges are perfectly synchronous. The amount to be taken away can be ascertained by experiment only; and as we approximate towards the truth we must be more cautious in using the file, and more accurate in making the observations. In this last step of the progress it is difficult in all cases to determine the exact quantity that has been filed away; and we may sometimes overdo it, so as to cause an inequality in the vibrations of an *opposite* kind, and thus render it necessary to *add* a small quantity to the end B. In order to meet this difficulty, Mr. Baily caused a small hole to be drilled at the end B, into which a screw was fitted; and by means of a small piece of sheet lead inserted underneath, the adjustment could be carried to any required degree of accuracy.

The principal advantages attending the placing of two knife-edges on one and the same bar, Mr. Baily states to be as follows: viz. 1st, That we are thereby possessed of two separate and independent pendulums, the results of which may be used separately or conjointly at pleasure; and each of which is a more complete check on the other than when formed of separate pieces of metal, that may probably be of different specific gravities, and of different expansive qualities. 2d, That the knife-edges, being once rendered synchronous, will always remain so, into whatever part of the world the pendulum may be carried, and thus enable us, if it should be required, to determine the length of the simple pendulum at any point of the globe at which it may be swung, by merely measuring the distances between the knife-edges. 3d, That we are thus furnished with the means of ascertaining whether the pendulum has sustained any accidental injury; since such a fact would be *immediately* discoverable from the inequality in the number of vibrations between the two knife-edges. And, even in case of such an unforeseen misfortune occurring, the *ratio* between the two would from that moment remain the same in all parts of the world, and answer the same useful purpose of *comparison* during the remainder of the voyage. Whereas, in the pendulum as usually constructed, the effect of such an injury (if not suspected) might be attributed to the errors of observation: and, indeed, the fact itself could not be ascertained until the return of the observer to some place where the pendulum had been previously swung; leaving, however, the precise *time* at

which the accident happened still undetermined, and not only the observations themselves, during some indefinite period, the subject of doubt and suspicion, but probably the whole series of no utility or avail.

Mr. Baily considers that there are also some advantages attending the *form* of this pendulum; for, being uniform in its dimensions, without any protuberant bob or projecting tail-piece, it is not so liable to accident as the ordinary pendulum, where those parts present opportunities for injury. It is also capable of being packed in a more convenient case, and thus rendered more easy of transportation, when required as a travelling instrument. But he states that if we view it in the light of an instrument intended for the observatory, as a means of determining the length of the simple pendulum, where both knife-edges are necessary for the solution of the problem, the advantages will be more apparent; for, in the construction of the pendulum here proposed, none of the parts slide over one another: there are no shifting weights, no movable screws: every thing is fixed and stationary, and consequently more peculiarly adapted for the determination of so nice and difficult a problem.

As these pendulums are formed without any tail-piece, it became necessary to adopt some other mode of determining the arc of vibration. This Mr. Baily has effected by making the edge of the pendulum, which is perfectly straight, answer the purpose of the point in the tail-piece of the ordinary pendulum: and by this method he was also enabled to enlarge the divisions of the scale (which is a diagonal one), so that the hundredth part of a degree occupies the length of three-tenths of an inch, and consequently can be read off with the greatest ease.

The reading of Mr. South's paper "On the Occultation of δ *Piscium* by the Moon, &c." commenced in April, was resumed and concluded.

Of all the phenomena which occupy the attention of the practical astronomer, the author of this paper considers that no one admits of such accurate observation as does the occultation of a fixed star by the moon; occasionally, however, a circumstance presents itself to his notice, which merits peculiar consideration, namely, an apparent projection of the star upon the lunar disk; the instances in which this anomaly has been observed in this country, are indeed rare, and this, he says, led many to consider it the attendant of a lively imagination. If, however, we dispassionately review the observations of continental astronomers, of unsullied reputation, there will, Mr. South says, be little reason to doubt the fact of apparent projection, although perhaps there may be considerable difficulty in arriving at its cause.

The only instance in which Mr. South has witnessed the phenomenon, was previous to the occultation of δ *Piscium*, on the 6th of February, 1821. (Latitude of the observatory 51° 30′ 2″,97 north, longitude of the observatory 21^s,76 west.) The night was beautifully fine, the moon's dark limb and unilluminated disk unusually

distinct, the atmosphere peculiarly serene, and the moon's limb, as well as the star, remarkably steady. The observation was made with his five-feet equatorial, furnished with a power of 127; the observer was at the telescope four or five minutes before the immersion could happen: every thing went on as usual, till the moon's limb came in contact with the star, but the expected occultation did not occur. He noted the time when the apparent contact took place, which was at $3^h\ 20^m\ 54^s,0$ by the clock. The star, unshorn of any of its splendour, remained visible on the unillumined lunar disk till $3^h\ 21^m\ 2^s,9$ by the clock, when it instantaneously disappeared. Not the slightest sensible deviation in the star's place occurred between the moments of apparent contact and subsequent disappearance; and it exhibited the same perfectly defined disk whilst on the moon's limb, as it was observed to have previously to the contact.

The corrections for the clock's error being applied, the observations will stand thus:

	h	m	s
Apparent contact at	3	20	29,87
Instantaneous immersion	3	20	38,77
Emersion	4	14	32,88

The only corresponding observations of this occultation which have come to Mr. South's knowledge, were made by Mr. Littrow and Mr. Baily, and their results are given in the Memoirs of this Society. As Mr. Littrow has not narrated any peculiarity, it is probable none presented itself to him; and Mr. Baily has authorised Mr. South to state that he saw nothing anomalous; a circumstance certainly important, seeing that in Blackman Street the apparent projection of the star on the moon's disk continued nearly nine seconds of time. On the same evening, in Blackman Street, a star of the 8th or 9th magnitude suffered occultation by the moon's dark limb, nearly at the same part at which δ *Piscium* entered on the disk; the star disappeared instantaneously at $5^h\ 2^m\ 6^s,0$ by Mr. South's clock: prior to occultation, however, this star was not seen projected upon the limb; but the low altitude of the moon rendered the observation less satisfactory than was the previous occultation of δ *Piscium*.

The recorded observations of other astronomers are then quoted in the words of their respective authors, or in authentic abstracts, and are principally derived from the *Mémoires de l'Académie Royale des Sciences*, the *Histoire et Mémoires de l'Académie Royale des Sciences de Toulouse*, the *Histoire Céleste Française*, the *Connoissance des Tems*, and the *Observations Astronomiques faites à l'Observatoire Royal de Paris*, tom. i.: this done, they are arranged in a tabular form, presenting, at one view, the name of the observer, the place of observation, the nature of the occultation, the age of the moon, and her motion at the time whether northerly or southerly; information of a nature not easily to be abridged, and far too voluminous to have insertion in the monthly notices of the Society's proceedings.

On perusing them, however, we find that more than twenty stars have exhibited peculiarites at or on the moon's limb, prior to im-

mersion behind it, or emersion from it; that the anomalies are not confined to stars of a certain magnitude or colour; nor are they dependent upon any particular age of the moon. Most of them have furnished but solitary instances of peculiarity; viz. *Spica Virginis*, *γ Libræ*, 132 *Tauri*, *α*[1] *Cancri*, *λ Aquarii*, 249 *Aquarii*, 187 *Sagittarii*, *γ Tauri*, *ϱ Leonis*, *ϱ Geminorum*, *δ Cancri*, and *δ Piscium*. One, *Regulus*, affords three, whilst to *Aldebaran* we are indebted for no fewer than twenty instances of anomaly.

On reference to the list, the anomaly alluded to, it will be seen, stands not upon the testimony of a single individual, but is supported frequently by the evidence of a second, and sometimes even of a third person; occasionally they are co-observers at the same station; at other times they are at different parts of the same city; whilst in some instances, they are separated by a very considerable distance. On the other hand, the conflicting testimonies, where we should least expect to find them, are perplexing; a circumstance which, together with the vague manner in which the observations are frequently recorded, and the habit which, in many instances, unfortunately prevailed, namely, of observing the immersion and emersion of the same star, on the same occasion, with different telescopes, and the almost constant omission to register, if the moon's dark limb *was* or was *not* visible, enable us, Mr. South says, to do little more than to state, with some appearance of probability, what are *not* the causes of the phenomena.

The hypothesis advanced as explanatory of the phenomena in question, are then stated: viz. a lively imagination on the part of the observer; a spurious disk given to the moon's image by the instrument of observation; a lunar atmosphere; irradiation; and lastly, different refrangibilities to which the rays from the moon and star are liable, arising from their differences of colour.

As unfavourable to the first hypothesis, which would refer the phenomenon to a lively imagination on the part of the observer, Mr. South advances the fact, that "more than sixty instances of anomaly stand attested by such men as Messier, Troughton, Bouvard, Arago, and Mathieu; and that it is rather too much to suppose that all of them are liable to the imputation which such an hypothesis would require.

The second hypothesis, which supposes a spurious disk to be given to the moon's image, might, he observes, be entitled to some consideration, had refracting telescopes, whose object-glasses were not achromatic, been solely employed for the observations; but seeing that refractors, long and short, achromatic and non-achromatic; reflectors, Newtonian and Gregorian, most of which were probably far above the rank of good instruments, and some of which certainly might be brought forward as the most perfect specimens of optical ingenuity,—have all exhibited the anomaly, there is considerable difficulty in receiving the hypothesis, unless indeed we could grant that a constant cause should not produce a constant effect.

The next hypothesis offers a lunar atmosphere as the occasion of

the apparent projection of a star on the moon's disc. Were this the case, its effects would be similar upon all stars of similar colour; and should we not see evidence of its existence, in some shape or other, at every occultation which occurred? yet, how infinitely few are the instances in which any thing whatever is observable of alteration in the star on the moon's approach to it, indicated by derangement of its position, diminution of its splendour, or change of colour. Still it must be remembered, such changes do stand on record; but they are either so unsubstantiated, or are so referable to other causes than a lunar atmosphere, that we are scarcely warranted in lending ourselves to the hypothesis, to which they would conduct us.

The hypothesis next in order suggests irradiation as the source from which the anomaly is derived. It seems, however, Mr. South thinks, incapable of answering the purpose for which it is brought forward; seeing that projections of stars upon the moon's *dark* limb have been witnessed by Messier, by Maskelyne, by Arago, by Mathieu, and by himself.

That the last hypothesis — namely, that which supposes the apparent projection to arise from the different refrangibilities of the rays issuing from the star and moon — is not tenable, Mr. South advances the circumstance, that not only *Aldebaran* and the *red* stars are liable to the anomaly, but that stars as remarkable for their *white* light as is *Aldebaran* for its *red*, have exhibited the phenomenon of apparent projection. He also says that, as far as he knows, no instance of apparent projection of the planet Mars upon the moon's disc, is at present recorded amongst the list of lunar occultations of that planet; yet *Mars* is much more decidedly *red* than *Aldebaran*, or than any other star which has been observed on the lunar disc.

Having, as he thinks, shewn that the above hypotheses are inadequate to the purpose for which they have been designed, Mr. South states that he should not be justified in advancing any hypothesis in addition to those which he has combated; but concludes his paper by stating, that from the *Connoissance des Tems*, he finds that the moon's path will, during the years 1829 and 1830, furnish several occultations of *Aldebaran;* when it is to be hoped that a phenomenon, which has been so little observed in Great Britain, that if it rested solely on the authority of British astronomers would be scarcely entitled to any notice, will not longer furnish an object for their reproach.

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